COPY

January 2, 1954

Professor Henry I. Kohn
Radiological Laboratory
University of California Medical Center
San Francisco 22, Calif.

Dear Dr. Kohn:

Thank you for your letter of the 27th. I am sorry that Dr. Adelberg misunderstood my comment about the distribution of E. coli W-478. Once having transmitted this culture, I had no intention of restricting or qualifying its further distribution, and I am pleased at your interest in it. I did mention that I would be curious to learn who else might be using diploids, and perhaps this is the basis of Dr. Adelberg's inference about the necessity for permission. I would be happy to collaborate with you if it were physically convenient, but it is difficult for me to see what I could do that you are not already well prepared for.

So, if the strains that Dr. Adelberg now has would suit your purpose there is no reason why you should not obtain them from him directly. I will be interested in the outcome of your experiments, for comparison with my own. If I can be of any direct assistance, please let me know.

I did some work on this problem myself some years ago, though the bulk of it was with ultraviolet. I was surprised to find that haploid and and diploid cultures had identical survival curves. I was also unable to obtain any concrete evidence for the occurrence of induced recessive lethals, which was the primary objective of the experiments. As best as I can recall now a few experiments with X-rays, in the dose range from 10 - 100 kr, gave very similar results. Radiations and other mutagens did have some remarkable effects on diploids. These have not been fully analyzed as yet, as the student who began to work on the problem was diverted by finding a phage-mediated transduction in the course of his preliminary experiments, and decided to pursue this instead. As far as our analysis went, we concluded that an irradiated ~~cell~~ diploid cell gave a clone in which, for several generations, there was a high probability of occurrence of haploid segregants and automictic (reorganized) diploids. The cell at the time of irradiation did not seem, therefore, to be the unit of response. A serious trouble in this, and related, analyses is the multinucleate character of the bacteria. An abbreviated account of these experiments was published in the Cold Spring Harbor Symposium for 1951-- see p. 428-430. I am hoping to go back to the question of recessive lethals at an early opportunity, but at the present time, I think that yeast (haploid and diploid with several heterozygous markers) is far and away the best experimental material for this kind of study.

Yours sincerely,

CC: Dr. Adelberg

Joshua Lederberg